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which he enumerates in some detail. In short, the book is so inaccurate that it is an outrage to put it in the hands of a single immature student. This book has been pushed by active and intelligent agents, and also widely advertised; it has doubtless been adopted in many schools. How have the scholars of America dealt with it? The first review I saw was in the *Nation*, of course anonymous. It was laudatory, and did not indicate that anything was wrong; though I remember a vague reference to some matters on which there might be differences of opinion. I wrote to the editor, pointing out the real character of the book, and received the reply that the reviewer quite agreed with me as to the work *as a text-book*, but reviewed it favorably because he thought it might be useful in other ways!

For some time no other review came to my notice, until I received the *American Breeders' Magazine*, Vol. 2, No. 1. Here, if anywhere, we might expect critical treatment. The review (p. 77) is wholly and extravagantly laudatory, without any hint of errors. It ends with the remark that "Dean Davenport's pioneering work is most valuable, both because of the excellence of his books and because they blaze the trail in this subject." The review is anonymous, and the editor, on being written to, does not defend it.

Finally, I find a review by Dr. Geo. H. Shull in *Botanical Gazette*, September, 1911. Dr. Shull, as might be expected, tears up and scatters to the four winds the treatment of Mendelism, but says that it lacks "the definiteness and accuracy which characterizes the rest of the book," and again "It seems unfortunate that a book otherwise so admirable should propagate such definitions as these."

I should have had something to say on this matter earlier, but for the fact that Ginn and Company's agent, visiting me here, gave me to understand that the edition would be withdrawn and a corrected one substituted. After a time, suspecting that this was not being done I wrote to the publishers direct and was told (August 21, 1911) that "no revision of it has been called for or made." A later letter

(September 4) stated that it was Professor Davenport's intention to make some changes and corrections which my earlier letters to the publishers had suggested. There is no indication whatever of any intention to withdraw the edition now on sale.

Other instances could readily be cited to show that vigilance is the price of accurate text-books. I will mention only one that came before me quite recently. Two books arrived in the same package from the American Book Company. One is Hunter's "Essentials of Biology," the other Sharpe's "Laboratory Manual for the Solution of Problems in Biology." The authors both teach in the De Witt Clinton High School. Hunter (p. 44) refers to the composite "flower cluster, so often mistaken for a single flower"; Sharpe (explanation to figure 6) does so mistake it, the legend reading "Curve of variation in number of petals of ox-eye daisy. . . . Number of petals to a flower on line *ac*."

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#### "AIR IN THE DEPTHS OF THE OCEAN"

TO THE EDITOR OF SCIENCE: In a recent number (August 25) an explanation is offered by Carl Hering as to the supply of dissolved oxygen, necessary for the respiration of fishes, even at great depths in the ocean. The suggestion is that the solubility of oxygen in water, being proportional to the pressure, is much greater at considerable depths than at the surface, and therefore the dissolved oxygen diffuses readily downwards.

There is surely a confusion of ideas here regarding pressure. The pressure to which the solubility of oxygen is proportional is the (partial) gas pressure of oxygen; the great pressure in the ocean depths is hydrostatic, which has but a very slight effect on the solubility of a gas.

The solubility of oxygen, therefore, does not appreciably increase towards the bottom of the sea, but the ordinary process of diffusion from the saturated surface layers may well provide adequate oxygen even at the greatest depths, in view of its uninterrupted action and the

length of time during which it has been in operation.

I have delayed submitting this note in the expectation that others would be as ready to convey information regarding air in water as they have been concerning water in air!

PERCY NORTON EVANS

LAFAYETTE, INDIANA,  
September 18, 1911

THE INFLUENCE OF HEREDITY AND OF ENVIRONMENT IN DETERMINING THE COAT COLORS  
IN MICE

PROFESSOR T. H. MORGAN,<sup>1</sup> in an interesting paper, has lately published the results of his breeding experiments with mice. Among other questions he considers certain curious coat patterns on black animals resulting from a black  $\times$  chocolate (brown) cross.

Such coat patterns, which appear to consist of well-defined regions of light and dark hair, he considers due to heterozygosis between the black and brown coat colors.

That such patterns are not due to heterozygosis of black and brown is, I believe, shown by the following three facts which I have been able to record:

1. That in mice, brown (chocolate) animals may possess these coat patterns while changing coats. These animals are by experiment proved to be free of all black pigment.

2. That in rabbits, black animals may show these coat patterns with extraordinary clearness. There is no brown (chocolate) rabbit recorded.

3. That the common gray squirrel frequently shows distinct coat patterns of this nature, when changing coats. This wild species is undoubtedly homozygous for its color pattern.

Morgan further suggests that these coat patterns in mice may be due to heterozygosis of intensity and dilution of coat pigmentation. This, I think, is disproved by the fact that I have obtained clearly defined patterns on the coats of dilute pink-eyed brown (chocolate) mice. These animals are the lowest recessives in the series of colored mice. They have been

<sup>1</sup> *Annals N. Y. Acad. of Science*, 1911, Vol. XXI., pp. 87-117.

found, by experiments, to lack the ability to produce black pigment, intensity of coat pigmentation and dark eyes.

It would seem then that the coat patterns recorded by Morgan as well as those mentioned above are the result of physiological conditions of the animals incidental to the coat-changing period, and that they can not be considered of any value as indicating the gametic composition of the animal on which they appear.

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October 5, 1911

QUOTATIONS

CONGRESS OF THE UNIVERSITIES OF THE BRITISH EMPIRE

A FURTHER meeting of the vice-chancellors of the home universities who constitute the Home Committee to make arrangements for the Congress of the Universities of the Empire, which is to be held in London next year on July 2, 3, 4 and 5, was held recently at the University of London under the chairmanship of Sir William Collins, vice-chancellor of that university. The meeting was also attended by Sir Charles Lucas, head of the Dominions Department of the Colonial Office; Sir Theodore Morison, a member of the Council of India; and Dr. Heath, of the Board of Education. In November last year an invitation was extended to the fifty-one universities in the British Empire to send representatives to the congress, accompanied by an intimation that the topics to be considered would fall under the following heads, but inviting suggestions: (1) University organization; (2) universities in their relation to teachers and undergraduate students; (3) universities in their relation to post-graduate and research work; and (4) universities in their relation to schools and to agencies for higher education. At the recent meeting the suggestions received from oversea universities were considered, and Dr. R. D. Roberts, secretary to the congress, made a report upon a preliminary conference of representatives of the Canadian universities, held at Montreal